Concluding Remarks:

PROSPECTS FOR THE STUDY OF PLANETARY RADIO EMISSION

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The Graz Workshop on Radio Emissions from Planetary Magnetospheres has been primarily concerned with the 'present' — observations and theoretical ideas of the past five to ten years, looking forward a few years into the future. I propose to discuss the future, but on a long timescale, extending forward twenty to thirty years.

The study of planetary radio emission in practice divides into two disciplines, ground–based measurements from a few megahertz upward in frequency, and space–based measurements that extend downward to electromagnetic waves at audio frequencies. Jovian noise bursts can be studied from the ground with relatively modest equipment, but more ambitious antennas of great collecting area can be envisioned. Space–based measurements depend upon targets of opportunity: Planetary missions that will accept the radio receivers and antennas, usually in a subsidiary role. The Voyager missions gave outstanding results for the four major planets, and Ulysses also yielded new results from Jupiter. The longer–term prospects are unclear, or perhaps too clear, since future planetary missions to the outer planets will be launched only rarely.

Radio studies of the planets have been marked by a continuous theme: The unexpected quality of the observed effects. This is only one aspect of the more general result that every time a planet is investigated closely by modern methods, new and surprising phenomena are discovered. The complexity of the observed radio emission follows from the rich variety of interactions that can occur within magnetoplasmas. In the more general context, our knowledge of cosmogony is imperfect, a truth that becomes more evident the more closely planetary phenomena are studied. Planetary radio noise in the framework of cosmogony has the capability of studing the magnetic environment of a planet and its interaction with the solar wind in our own solar system, and potentially with stellar winds in other solar systems. It may even enable the measurement of planetary magnetic fields in other planetary systems – what might be termed exoplanetary systems.

The latter possibility, of studying the properties of other solar systems, would appear, at first glance, to offer only a remote chance of success. There are two hundred thousand astronomical units per parsec, so fluxes would scale by a factor of the order of 10^{10} or 10^{11} for the nearest stars. Nevertheless, a quantitative examination shows that the prospects are by no means hopeless. For example, there are about 100 stars within ten parsecs of

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the sun. The closest is the α Centauri system, a multiple–star complex at a distance of one parsec. The prospects for forming planets in multiple–star systems are supposed to be poor, but a close look at the problem shows that there are well–defined regimes within which planets can have quasi–stable orbits. Our uncertain knowledge of cosmogony should certainly encourage a study of these nearby stars. The closest single star of solar–type is τ Ceti, a G5V star at a distance of 3 parsecs. If planetary systems are a common occurrence, a not unreasonable assumption, this system should offer excellent prospects for exoplanetary studies. Within ten parsecs, there are at least twelve non–binary stars of roughly solar type.

The largest Jovian decametric bursts exhibit a flux in the range 10^{-15} to 10^{-16} $\mathrm{Wm^{-2}s^{-1}Hz^{-1}}$. If there is a Jupiter–like exoplanet in orbit about τ Ceti, its strongest bursts would give a flux of a few Janskys, given the inverse–square factor of 3×10^{10}. There are already radio telescopes in existence (such as the Arecibo and Kharkov instruments) that have a collecting area of the order of 10^5 m^2, and at decametric wavelengths, these have a detectivity factor $T/A \approx 1\mathrm{Km^{-2}}$.

With proper signal processing, the strongest decametric Jupiter–like bursts should be detectable with these instruments. An exploratory search might be considered at the present time, especially when one recalls that we know very little about the total range of possible planetary radio phenomena. One can consider, for instance, the following scaling laws for the energy E available from a region of size L, electron density n_e, electron velocity β_e, electron energy ϵ, and magnetic field B:

$$E \sim \begin{cases} L^3 n_e \beta_e^2 B^2 & \text{(linear processes)} \\ L^3 n_e \epsilon & \text{(non–linear processes)} \end{cases}$$

One cannot say, whether conditions at such an exoplanet would give decametric radio bursts stronger or weaker than those of Jupiter, but much stronger radio bursts might well be the case. The available energy given by the second formula above, the non–linear case, might well turn out to be almost fully utilizable for radio noise production, if the process has high efficiency. Our experience so far seems to indicate that nature frequently follows a course that might be described as the ‘principle of strong relaxation’. If simple linear radiation mechanisms are inefficient, but a collective mode of radiation exists, then nature will choose the collective mode whenever possible. Interstellar masers of very high efficiency are commonly observed in the line radiation from hydroxyl radicals, and from many other molecules such as water, methyl alcohol, and silicon dioxide. Cyclotron masers appear to be responsible for much of the decametric radio phenomena from planets. One has, therefore, grounds for optimism. Twenty or thirty years ago, the suggestion that one could observe exoplanetary magnetospheres and infer the values of the planetary magnetic fields would not have been taken seriously, but it appears that such a new direction could be open even with instruments that exist today. A decametric radio telescope with a collecting area of a million square meters – a square kilometer – is by no means an impossibility, and would surely be capable of studying decametric radio emission from a number of nearby exoplanetary systems.

The prospects for studying radio noise from exoplanetary systems should also improve with a new NASA initiative, known as the TOPS Program (Towards Other Planetary

Systems). The program is broadly conceived to study exoplanetary systems in all their aspects. This would include the study of pre–planetary and circumstellar disk phenomena, recognizing that between the formation of stars (the concern of astrophysics) and planetary science (the concern of a broad range of planetary disciplines) there is a potential administrative gap. That area, if neglected, could well result in missing the data that would bear on the central questions of cosmogony. The eventual aim is to conceive and encourage missions that would allow the study of the physical and chemical conditions of exoplanetary systems in the most general way.

The TOPS Program currently has a three phase formulation: The first phase, covering the next ten years or so, would emphasize ground–based studies of preplanetary and circumstellar disk phenomena, particularly in the infrared, and the planetary search aspects would utilize indirect detection methods such as astrometry and radial velocity techniques. The goal of the planetary search program would be to find one or a few Jupiter–like planets. Technology development for the later, more ambitious space missions would also proceed during this phase. The second phase would concentrate on the development of a mission that would be capable of detecting a large sample of Jupiter–, Uranus– and Neptune–like planets in addition to studying circumstellar disks with high angular resolution. The new start would occur in the late 1990's. The third and most ambitious phase would be centered on the construction of a very large interferometric system that would be capable of carrying out direct detection of planets, including terrestrial planets. It appears that an instrument capable of detecting exoplanets directly would be able to carry out physical and chemical studies of potentially deep implications.

The third phase depends upon space policy decisions that have not yet reached final form. One of the most attractive possibilities would be to build the instrument on the surface of the moon, using the support facilities that could be provided by a permanent lunar base. The timescale for such a development appears to be at least twenty years, but a large–scale interferometer for the optical, infrared, and sub–millimeter domains of the spectrum would be a natural project to be supported by such a facility.

Presently, exoplanetary radio emissions have only been considered in the context of millimeter and sub–millimeter radiation from circumstellar disks and pre–planetary nebulae. The potential for studying exoplanetary magnetic fields and their associated magnetospheres would appear to be a legitimate part of an exoplanetary study program. The considerations above indicate that decametric studies can probably be made from ground–based facilities, but at hectometric radio frequencies and below the prospects for ground–based observing seem to be unpromising because of terrestrial radio interference. A low–frequency array on the moon might be a serious contender for a lunar–base facility. Whether the array would be built on the near side of the moon (depending upon ionospheric shielding to exclude interference) or on the far side of the moon (which should be a pristine environment) would depend upon the results of detailed mission studies.

The underlying promise of exoplanetary studies has already been stated: Every planet presents surprises. It is not impossible to imagine that interesting radio emission at low frequencies may be observable from pre–planetary systems as well. One should recall that there are a host of puzzling problems associated with the formation of our own solar system, such as the evident presence of ^{26}Al and ^{244}Pu during the planetary formation process. We also know that the OH interstellar masers give good evidence of magnetic fields of the order of 5 milligauss over dimensions of 10^{15} centimeters. As planets form in such a medium, these magnetic fields might well strengthen to much larger values, and the resulting electromagnetic phenomena could be most dramatic. The closest stars with preplanetary disks are the T Tauri stars and there are several such complexes within 100 parsecs. There is good evidence that they possess dense disks for the first million years of their lives. Given the possibility of strong magnetic fields and the strong relaxation principle commented on earlier, detectable radio emission at low frequencies is not unlikely.

The scenario for such developments can probably unfold in a logical fashion. The Arecibo and Kharkov instruments can be used now to explore the prospects, and if positive results should be forthcoming, this would almost certainly justify the construction of a much larger dedicated low–frequency array. The extent to which this would justify the construction of a lunar array would depend upon the character of the results from the ground, but one does not have to be too much of an optimist to realize that the electromagnetic phenomena exhibited in magnetoplasmas are a rich and only partially explored field. The study of radio emission from the planets of our own solar system has been fruitful, and there is good reason to believe that the results of similar exoplanetary phenomena would be equally rewarding.

(Postscript:
In a later discussion with Dr. George Field of Harvard, we reviewed the potential results that might come from a successful detection and subsequent study of decametric radiation from an exoplanet. Given a sufficiently large collecting area, the rotation period of the object would certainly be measured. In addition, over time not only the period but the frequency modulation resulting from its orbital Doppler shift should be detectable, thus leading to the determination of the orbital size.)